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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A comprehensive investigation on afternoon–evening transition of the atmospheric boundary layer over a tropical rural site" *by* A. Sandeep et al.

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The discussion paper presents a variety of observations of afternoon transitions. The observations are of good quality. Analysis at a tropical site adds something to the literature, which mostly has looked at mid-latitude situations. I think the paper could be a useful addition to the literature, but some aspects of the presentation need to be improved before publication.

General comments:

1. The paper introduces a new term, "afternoon-evening transition." Transition termi-

nology is already confusing enough. Please choose a term from the Lothon et al. paper.

2. Since this is a tropical site, something should be said about how it is or should be different than a mid-latitude site. For example, does the smaller range of solar zenith angles matter to the range of transition times? What about the more rapid reduction of incoming radiation at lower latitudes?

3. By the time availability of data from all platforms is taken into account, the number of days included in each section of the study is small and different. Please be more clear about this, even to the point of being tedious and repetitive. A related point is that the filtering for clear-sky days must introduce important biases especially in the monsoon seasons. Again, this needs to be made very clear.

4. The transition times are apparently chosen subjectively and are necessarily somewhat uncertain. This is not a problem, but should be made completely clear.

5. The word "collapse" should be removed everywhere it occurs, since the paper shows that it is an inappropriate way to think about the transition.

6. The entrainment flux analysis starting on p.31498 is interesting but difficult. This has been attempted previously but with no success. The main difficulty is obtaining meaningful measurements of the very small mean vertical velocity. Advection, which is not included in eq.3 but should be, is also usually important and very difficult to estimate. The results presented in figure 6 seem reasonable, but in order to give readers confidence that they are in fact correct, much more information is needed. A detailed uncertainty analysis should be done and error bars put on the fluxes. Some justification for the neglect of advection is also needed. If this harms the flow of the paper too much, it could be put in an appendix or supplement, but it must be available to interested readers. Finally, figure 6 c and d are confusing because the days are run together as if they were continuous. At least the lines should be broken between the days, but a separate, larger figure might be better.

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7. A related point to the above is that, as shown here, the concept of entrainment ratio has limited applicability and should be used with caution.

8. In general the figures need to be bigger and more readable. Not all of this is under the direct control of authors, but I urge the authors to work with the journal staff to make readable figures.

Specific comments:

1. Abstract, second paragraph: The wording is unclear. In fact the first evidence of the transition is aloft in the profiler data, followed by the sodar data and then the surface.

2. It seems that the SNR plots are not range-corrected. Is this true, and if so, why not? Plotting range-corrected SNR is clearer and more customary.

3. p.31495, top: Is there really not a consistent pattern between the radar and sodar? Elsewhere it is asserted that there is a significant difference in timing.

4. p.31495 line 3 and p.31496 line 21: Why is it considered easier to use SNR than sigma? It looks to me like sigma is even clearer than SNR.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31483, 2014.